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Does limited access at school result in compensation at home? The effect of soft drink bans in schools on purchase patterns outside of schools

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Abstract

This paper investigates the effects of soft drink bans in schools on purchases outside of schools. Using unique household-level data, we exploit the implementation of a state-mandated ban on soft drinks in Connecticut (USA) in a triple difference approach. We compare soft drink purchases of households with school-age children before and after implementation with purchases of households without school-age children in Connecticut, as well as households with and without school-age children in other states. Our analysis does not support the notion that school-age children compensate for the limited availability at school with increased consumption at home.

Keywords: soft drink bans, purchase data, school environment, quasi-natural experiment

JEL classification: D01, D12, D18, C93

1. Introduction

During the past three decades, childhood obesity has more than tripled in the USA.[1] Childhood obesity is associated with health problems at a young age, such as type 2 diabetes, cardiovascular diseases and asthma (American Heart Association, 2008). Increases in total caloric intake play a critical role in the growth of obesity, with soft drink consumption identified as one of the major contributors (Vartanian, Schwartz and Brownell, 2007; Brownell and Frieden, 2009). The school environment–its physical, social and educational surroundings–has become a focus in the public policy debate in this context. A number of

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1 The prevalence of obesity among children aged 6–11 years increased from 6.5 per cent in 1980 to 19.6 per cent in 2008, and the prevalence rate among adolescents aged 12–19 years increased from 5.0 to 18.1 per cent (Centers for Disease Control and Prevention, 2011).

states have considered mandatory policies directly addressing soft drink availability in schools as a nutritional consideration (Center for Science in the Public Interest, 2007), and national mandatory guidelines are currently discussed in the USA. In addition, the Alliance for a Healthier Generation reached an agreement with the beverage industry, setting voluntary guidelines to shift to lower calorie, more nutritious beverages (American Beverage Association, 2010). Our study informs the ongoing policy debate by investigating the effects of soft drink bans in schools on out-of-school soft drink purchases. This focus addresses shortcomings in the existing literature and provides a first insight into whether restricted access at school can ultimately reduce children's *overall* soft drink consumption.

Federally reimbursable school breakfast and lunch programmes must meet stringent nutrition standards under the National School Lunch Program (NSLP) and restrict availability of soft drinks during breakfast and lunch hours. Yet, two-thirds of states have weak or no nutrition standards for competitive foods.[2] Proponents of regulations beyond the NSLP state that providing healthy snacks and limiting access to foods of minimal nutritional standard will improve children's diets because children will consume foods and beverages that are most easily available to them. Support for this position comes from related research indicating that people eat more when they are provided with easy access to food (e.g. see Wansink, 2004; Geier, Rozin and Doras 2006; Rolls, Roe and Mengs, 2006). Restricting access should therefore reduce consumption. Opponents fear a loss in revenue and argue that children will compensate by consuming more soft drinks at home (e.g. see Heatherton, Polivy and Herman 1990; Fischer and Birch, 1999; Francis and Birch, 2005).

To date, there is little direct evidence for either position (Rudd Center for Food Policy and Obesity, 2009). Studies on the effect of improved nutritional choices and/or educational campaigns rely mainly on survey responses and small sample sizes and primarily focus on elementary schools (e.g. James *et al.*, 2004; Blum, Jacobsen and Donnelly, 2005; Fernandes, 2008). While these studies report moderate decreases in soft drink consumption at school, a study addressing high school consumption in Maine finds very limited effects on beverage choice of students (Blum *et al.*, 2008). Schwartz, Novak and Fiore (2009) suggest that removing low-nutrition foods decreased students' consumption at school, and detect no compensation effect at home based on self-reported purchase behaviour by students. Our study, the first one to our knowledge that analyses actual out-of-school purchases contributes to this literature by directly addressing whether banning soft drinks at schools results in compensation effects at home.

We use unique household-level purchase data from the AC Nielsen Homescan. These data allow us to directly link actual purchases to household

2 Competitive foods, often of little or no nutritional value, are those which compete with federally regulated school meals programmes, and are sold in vending machines, school stores, cafeteria a la carte lines, and at fund raisers.

demographics in a reduced-form econometric approach. We utilise a quasi-natural experiment and build on triple difference (DDD) specifications in a treatment framework commonly used in the policy evaluation literature (see Gruber, 1994; Meyer, 1995; Bertrand, Duflo and Mullainathan, 2004). During our data period, Connecticut implemented a complete ban on all regular and diet soft drink products sold in public schools effective from 1 July 2006. We compare soft drink purchases of households with school-age children in Connecticut with purchases of households without school-age children in Connecticut, as well as households with and without school-age children in other states without state-level regulations. Pre- and post-ban time periods allow us to separate the effect of the soda bans from household-specific, time-invariant unobserved factors that might be correlated with soft drink demand. Furthermore, we are able to test if leading soft drink manufacturers intensify their advertising efforts to school children in the states that have implemented bans by controlling for potential differences in children's advertising exposure.

Overall, our analysis does not support the notion of compensation effects in out-of-school household purchases.[3] As such, our results suggest that banning soft drinks at schools as one possible restriction of access to unhealthy foods and beverages might be a viable policy option in an attempt to reduce childhood obesity.

The next section of this paper briefly reviews soft drink bans and related regulations in the school food environment in the USA. Section 3 describes our empirical setting by describing our data, research design and econometric specifications. We discuss our results and robustness checks in Section 4 and conclude with a discussion of our findings and further research directions in Section 5.

2. Soft drink bans and regulations in the school food environment

Credible estimation of treatment effects in our empirical strategy relies on correctly defining a treatment and control groups in our empirical framework. We therefore conducted a comprehensive review of existing policies, using the yearly update and overview provided by the National Conference of State Legislators (2007), cross-checked available local government and school district information and searched local and national media to detect potential related interventions at the city, school district and school level.

California was the first state to introduce and pass state-level regulation, banning soft drinks from elementary, middle and junior high schools (except for special events) in 2004. In 2006, California further modified the beverage restrictions to require soft drink bans at high schools with at least

3 We do not observe restaurant purchases as another possible outlet for out-of-school purchases of soft drinks, a caveat that will be discussed in more detail in Section 3.2 and in the conclusion of this paper.

50 per cent compliance by 1 July 2007 and 100 per cent compliance by 1 July 2009. In the same year, Connecticut passed a law that banned soft drinks sold to students in all public schools starting 1 July 2006. A number of other states have set nutritional guidelines, proposed and passed related measures, but have not passed state-wide bans. In addition, the Alliance for a Healthier Generation (a partnership of the American Heart Association and the William J. Clinton Foundation) and beverage industry representatives reached an agreement for voluntary guidelines to shift to lower calorie, more nutritious beverages for children's consumption during the regular and extended school day. The industry fully implemented these guidelines on a voluntary basis by the 2009–2010 school year. And finally, restrictions were also implemented at the city and school district level. For instance, Baltimore prohibited sales of foods and beverages with minimal nutritional standard (including soda) starting in September 2006, while carbonated beverages were not sold in school vending machines in Detroit starting on 31 December 2005. The Philadelphia school district further approved a soft drink ban, effective from 1 July 2004 for kindergarten to 12th grade levels (K12). Our analysis focuses on the Connecticut state-wide ban. The regulations used to define our empirical setting are summarised in Table 1.

3. Empirical setting

3.1. Data

Our data consist of a geographically and demographically representative sample of household panel purchases (Nielsen Homescan) covering three years (from January 2006 to December 2008) in 16 geographical markets or designated marketing areas (DMAs). The data contain price, quantity and promotional information on transaction-level household purchases of soft drink products at the universal product code level from all shopping outlets (e.g. grocery stores, drug stores, vending machines and on-line stores).[4] The data also include annual demographic information for each household, such as income, race, household size, education, employment, occupation of household heads and, most importantly for our study, age and presence of children.

Due to the increased use of these data in academic research, recent papers have discussed potential caveats of the Nielsen Homescan panel. Einav, Leibtag and Nevo (2010) match the Homescan panel to transactions recorded by a large grocery retailer. They find discrepancies in reported shopping trips, products, prices and quantities, with the largest discrepancies in the price variable. Zhen *et al.* (2009) further suggest potential systematic underreporting of food expenditures, and Lusk and Brooks (2011) discuss potential sample selection. However, the advantages of the Nielsen Homescan panel data are that these data do not rely on consumer recall as they track actual purchases,

4 The Nielsen Homescan instructs its panel members to use in-home scanners to record all purchases from any outlet that are intended for personal consumption by any household members.

Table 1. The regulatory environment

Year	Stringent regulation (soft drink ban) implemented	Less stringent regulation (addressing soft drink availability) introduced
2004	California (elementary and middle schools); Philadelphia	Washington State; Louisiana; Tennessee
2005		Arizona; Kansas; Maine; Maryland; New Mexico; North Carolina; Texas; South Carolina; Utah; Rhode Island; Louisiana; Oklahoma; West Virginia
2006	California (50 per cent compliance in high schools by July 2007); Connecticut (all schools); Baltimore; Detroit	Indiana; New Jersey; Rhode Island; Mississippi
2007		Mississippi; North Carolina; Oregon; Rhode Island
2008		Colorado; Massachusetts
2009	California (100 per cent compliance in high schools)	
2009	Beverage industry voluntary guidelines to shift to lower calorie options (all schools nationwide)	

Notes: Connecticut and California implemented stringent soft drink bans at schools. Other states introduced less stringent regulations, such as restrictions for school meal hours, for instance. Based on the DMAs included in our data, we also reviewed potential city-level soft drink bans in these states, eliminating Detroit and Baltimore as potential non-experimental DMAs. Based on this policy review and our data coverage, we use Hartford, CT, as our experimental DMA. Atlanta, GA; Houston, TX; Miami, FL; and Kansas City, Mo are included as non-experimental DMAs in our analysis. Due to the phase-in of the ban in California, and the limited time series overlap in our data, California DMAs were not included in this analysis.

and include purchases from retailers that do not cooperate with scanner data collection agencies such as Walmart and Whole Foods. They allow us to directly link actual purchases to household demographics in order to define our treatment and control groups. For this study, we focus on the quantity purchased rather than the price data. And finally, the limitations discussed above should not affect or apply differently to households in our defined treatment and control groups such that our analysis of differences remains valid.

We further combine these household purchase data with the Nielsen Media data set. It consists of brand-level television advertising information for each of the 16 DMAs and all soft drink products covered by the Homescan data (taken at weekly intervals). The advertising data set is unique in that it includes not only brand-level advertising expenditures, but also advertising exposure measures for each brand, and five age groups at the DMA level. Specifically, advertising exposure is measured by a gross rating point (GRP) on cable, syndicated, network and spot television for audiences aged 2–5, 6–11, 12–17, 18–24 and over 25 years.[5]

5 GRP is the percentage of an audience in a given population reached by a specific advertisement over a specific week. It is the sum of all rating points, where a rating point of an advertisement is

3.2. Research design

The school environment – its physical, social and educational surroundings – provides an appealing case for policy interventions addressing children's eating habits. Yet, these policies aim to affect children's consumption of food and beverages beyond school hours and grounds. If we find that banning soft drinks in schools leads to no change or even a decrease in out-of-school soft drink purchases by households with school-age children, we can reject the argument that children compensate for reduced soft drink availability at schools. We can then argue that it is very likely that overall soft drink consumption went down as a result of these policies. While we do not directly observe consumption during the school hours, this conclusion would rely on results found in previous studies focusing on school purchases only (e.g. Schwartz, Novak and Fiore, 2009; American Beverage Association, 2010). A potential caveat in this argument is that we do not observe soft drink purchases in restaurants, such as fast food outlets. While food expenditures and calorie intake from food away from home (FAFH) has increased for both adults and children,[6] consumption of FAFH may not be a direct cause of weight gain. Instead, higher consumption of FAFH might be a result of family time constraints, access to various food outlets and preferences for certain foods (Mancino *et al.*, 2010). Therefore, we would expect to detect a compensation effect in our data sources as well, even though we would underestimate the overall compensation effect due to potential purchases in other outlets.

In order to credibly test for potential compensation effects, we exploit variations in soft drink bans over time, across different states, and the fact that these bans should only affect households with school-age children. Our reduced-form econometric approach builds on difference-in-differences (DID) and DDD specifications commonly used in the policy evaluation literature. Estimation of average treatment effects (ATEs) in this framework rests on the assumption that average differences in outcomes for treated and control groups are attributable to the treatment, which is satisfied when treatment assignment and the potential outcomes are independent (Imbens, 2004).

Our research design makes use of a quasi-natural experiment. Connecticut banned soft drink in all public schools, effective from 1 July 2006. The Hartford DMA in Connecticut therefore serves as the experimental DMA in our

the percentage of households watching a particular programme, relative to the total number of households with television sets in a DMA. That is, if the advertisement has a rating of 7, then 7 per cent of all households who have television sets in this DMA tune in to this commercial. If an advertisement is aired twice during a week, and has a rating of 7 and 10, respectively, then its GRP for that week is 17.

6 In 1977–1978, the average child aged 2–17 obtained 20 per cent of his or her daily calories from FAFH, while analysis of 2003–2006 data from the National Health and Nutrition Examination Survey (NHANES) finds that children get roughly 35 per cent of their calories from FAFH (Mancino *et al.*, 2010). School breakfast and lunch programmes as well as food purchases at school are included in these measurements, however.

research design. Based on our comprehensive regulatory review, we select Atlanta, Houston, Miami and Kansas City as the non-experimental DMAs. To our knowledge, these cities have no state, city or school district-level soda bans in place.[7] Furthermore, we define our potential treatment group as households with school-age children (aged 6–18). The control group consists of households without children, or without children aged 6–18. In order to address the fact that soft drink purchases are highly seasonal and isolate the treatment effect from seasonal effects, we choose the same months in the years as pre- and post-ban periods when schools are in session. The pre-treatment period is therefore defined as the four-month period between February and May in 2006, and the post-treatment period is defined as the four-month period between February and May in 2007.

Due to the quasi-natural character of this experiment, we have to consider a number of potential endogeneity sources in our research design and econometric analysis. First, although childhood obesity rates in Connecticut are similar to national averages prior to the ban,[8] it is possible that the Connecticut ban is endogenous to soft drink consumption. That is, some unobserved factors might be correlated with both household soda demand and the passage of the ban in Connecticut. For instance, consumers could be more health conscious in Connecticut when compared with other states, resulting in both passing of the regulation and decreased soft drink consumption.[9] Following Gruber's (1994) language, our implementation of the DDD model addresses this issue in three ways. First, we use pre-treatment and post-treatment period fixed effects, as well as month and year fixed effects to capture any trend in soft drink purchases that are common to all DMAs. Second, we use household fixed effects to control for any time-invariant household-level differences that could contribute to soft drink consumption. And finally, in order to control for potential time-varying factors within DMAs potentially correlated with the policy implementation and soft drink consumption in the experimental DMA, we compare households with school-age children in the experimental DMAs with households without school-age children in the same DMA. We measure the change in the treatment household's relative soft drink purchases in the experimental DMA, and relative to the non-experimental DMAs. And finally, we also control for potential time-varying factors common to all households with school-age children, comparing

7 Due to the partial introduction of the California ban during our data coverage, California DMAs are not included as experimental DMAs in the analysis. One of our colleagues further suggested a soft drink ban in Miami during our estimated time period. While we could not verify that information at the state level, we excluded Miami as a non-experimental DMA, as an additional robustness check.

8 Approximately 12.3 per cent of the Connecticut children were overweight or obese relative to a national average of 14.3% based on the National Survey of Children's Health 2003 (US Department of Health and Human Services, 2005).

9 Another form of endogeneity in this context would arise if households move in or out of the states because of the bans. Households would therefore self-select in or out of our treatment, ultimately biasing our results. Using the annual demographics data, we examine whether we see any abnormal migration patterns after the implementation of the ban, but do not detect any.

households with school-age children living in the experimental DMA with those living in the non-experimental DMAs.

The resulting identification assumption of the DDD is fairly weak. It only requires that there is no contemporaneous shock on households in the experimental DMA in the post-ban period that affects the relative outcomes of the treatments. In other words, identification of the ATE of soft drink bans would be violated by any systematic shocks to soft drink purchases of households with school-age children in Connecticut that affect soft drink consumption over time and might be correlated with but not caused by the ban. One possibility relates to the fact that soft drink manufacturers might attempt to compensate for the loss of sales and visibility at schools by intensifying local advertising campaigns directed at school-age children in Connecticut, and as a result, households with children might increase their purchase of soft drink relative to households without children. Our data allow testing this hypothesis as we are able to combine household purchases with DMA-specific time-varying and age group-specific advertising exposure.

3.2.1. Econometric specification

We obtain our sample from the Nielsen Homescan household-level purchase and advertising data described in the Data section.[10] While some households entered or exited during the middle of our data period, our sample only includes households who were in the panel during both the pre- and the post-ban periods.[11] For our analysis, we collapse the monthly time series data into two data points for each of these households, one for the pre-treatment and one for the post-treatment period. This approach closely follows Bertrand, Duflo and Mullainathan (2004) and corrects for artificially low standard errors in the presence of serially correlated outcomes in panel data sets. Specifically, for each household, we compute the average monthly volume of soft drinks purchased in each of the two data periods.[12]

We first specify and estimate the following DID equation:

$$y_{it} = \beta_0 + \beta_1 x_{it} + \beta_2 \tau_t + \beta_3 (CT_i \times \tau_t) + \beta_4 \mu_i + \varsigma_{it}. \tag{1}$$

10 Although the Nielsen Homescan is a representative sample, our selected regression sample might not be representative. Households in the Nielsen panel are assigned a ‘projection factor’, which represents the weight of the household in the national population. In constructing the regression sample, we weigh each of the households equally in our regression. In unreported regressions, we weigh the households with these ‘projection factors’ as additional robustness checks. This approach did not alter our results reported here.

11 We do not directly observe whether a household is included in the panel or not at any given time so we can only indirectly infer this. We have household purchases in four frequently purchased packaged food categories: soft drinks, breakfast cereal, snacks (such as potato chips, nuts or popcorn) and candy and confectionary. We keep only households who purchased any product in these four categories during both periods. Also, demographics are reported for each year a household is included in the panel. We compare the demographics and locations of households who were in the panel during both periods with those who entered late or exit early. We find no statistically significant differences and conclude that sample attrition appears to be random.

12 Not all households purchased soft drink products in both periods such that the volume of purchase of a household in a period could either be zero or positive.

Households are indexed with indexes time period (taking the value of 1 if it is in the post-treatment period, and 0 otherwise). Therefore, y_{it} is defined as the average monthly soft drink volume purchase by household i at time t. The intercept common to all households in all periods is denoted by β_0 and τ_t denotes the post-ban indicator that takes on the value of 1 if we are in the post-ban period, and 0 otherwise. The time period fixed effect controls for trends in monthly volume soda purchase that is common to all households in all states (β_2). CT_i is a Connecticut fixed effect which takes the value of 1 if household i lives in Hartford, the Connecticut DMA. We also include a number of observable control variables such as household demographics, prices and advertising exposure denoted by the vector x_{it} and time-invariant household fixed effect, μ_i.[13] The interaction between the Connecticut indicator and the post-ban dummy (β_3) is the DID estimate of the effect of the Connecticut soft drink ban on out-of-school soda purchases (average monthly volume) for Connecticut households. It captures the change in volume purchase by Connecticut households (relative to households in other states without soft drink bans) during the post-ban period (relative to pre-ban period). And finally, ς_{it} denotes an idiosyncratic disturbance term.

The DID identification relies on the common trend assumption. That is, in the absence of the soft drink ban, the unobservables that are correlated with soft drink volume purchased by Connecticut households follow similar time trends as for households living in other states. For instance, the common trend assumption is not likely to hold if households with school-age children follow a different trend than households without school-age children. We can additionally exploit the fact that the Connecticut ban should only affect households with school-age children in that state and utilise a DDD framework which allows relaxing this assumption. Specifically, we estimate the following DDD equation:

$$\begin{aligned} y_{it} = {} & \beta_0 + \beta_1 x_{it} + \beta_2 \tau_t + \beta_3 \mathrm{Treat}_i \\ & + \beta_4(\mathrm{CT}_i \times \tau_t) + \beta_5(\tau_t \times \mathrm{Treat}_i) + \beta_6(CT_i \times \mathrm{Treat}_i) \\ & + \beta_7(\tau_t \times \mathrm{CT}_i \times \mathrm{Treat}_i) + \beta_8 \mu_i + \varsigma_{it}. \end{aligned} \tag{2}$$

Here Treat_i indicates a household i that has children aged 6–18 in either Connecticut or the non-experimental DMAs, and zero otherwise and other notations are the same as in equation (1). The treatment group fixed effect controls for time-invariant characteristics of households with children aged 6–18 (β_3). The second-level interactions control for changes in volume purchase trends over time common to all households in Connecticut (β_4), changes in trend over time for treatment group households in all states (β_5) and time-invariant characteristics of the treatment group in Connecticut

13 There are no DMA level fixed effects in the specification because only less than 0.1% households in our data moved from one DMA to the other DMA between the two periods and we exclude these households from our analysis. Hence, DMA fixed effects are perfectly collinear with household fixed effects.

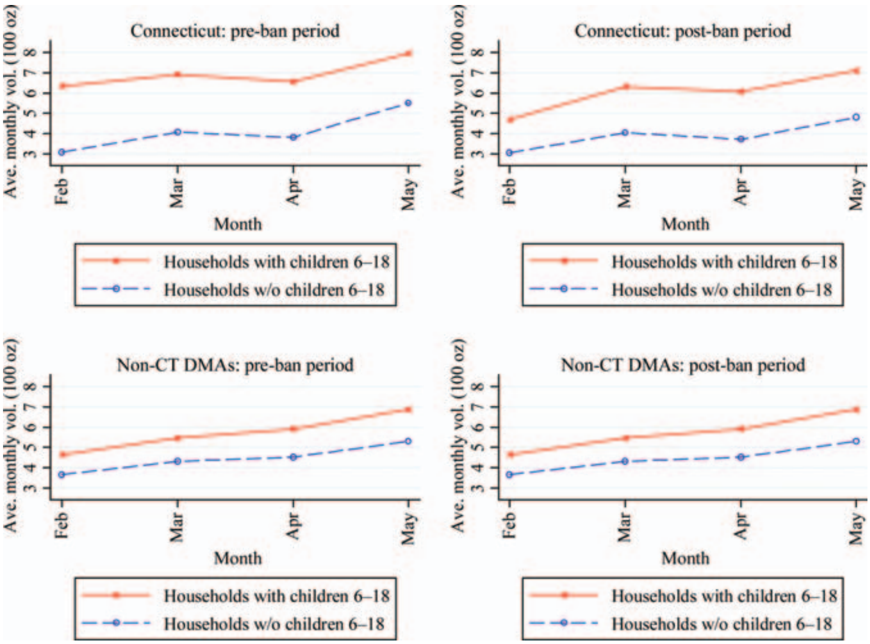

Fig. 1. Effect of CT ban on at-home soft drink purchases. Notes: Contains the graphical analysis for the Connecticut (CT) soft drink ban. The treatment is the CT ban on soft drink from all public schools effective from 1 July 2006. The pre-ban period is defined as February to May 2006, and the post-ban period is the same months in 2007. The treatment group includes households with children aged 6-18 only, and the control group includes households without children aged 6–18. The experimental DMA is Hartford, CT. The non-experimental DMAs are Atlanta, Houston, Miami and Kansas City. The two top panels depict the average household monthly soft drink purchase in volume (100 oz) by the treatment and the control group in Hartford, CT, during the pre- and the post-ban period, respectively. In the two bottom panels, we plot the same series for treatment and control groups in the non-experimental DMAs.

(β_6). The third-level interaction (β_7) is the DDD estimate of the effect of the soft drink ban on out-of-school soda purchases (average monthly volume) for households with children aged 6–18 in Connecticut. It captures the change in volume purchase by households with school-age children (relative to households without school-age children) in Connecticut (relative to households in non-experimental states) during the post-ban period (relative to pre-ban period). As in equation (1), we cluster standard errors at the household level.

4. Results and robustness checks

One advantage of the DDD approach is that graphical analyses can reveal the existence (or the lack) of treatment effects. Figure 1 illustrates this for the Connecticut ban study. In the two top panels of Figure 1, we show the average monthly soft drink purchases (in volume) for households with and without school-age children in Hartford, CT (experimental DMA), for the pre- and post-ban period, respectively. The two bottom panels show their counterparts for the non-experimental DMAs combined. We notice that in both experimental and non-experimental DMAs, the volume purchases are notably lower for both the potential treatment (households with school-age children) and the control groups. In all panels, the volume purchases by households with school-age children are higher, but the gap between the potential treatment and control households seems similar in the pre-ban period across the experimental and non-experimental DMAs. The graphical analysis seems to suggest that this gap remains unchanged in the non-experimental DMA while it might be even narrow in the experimental DMA for the post-ban period. We therefore do not expect to detect compensation effects in the Connecticut study.

Differences in changes of soft drink marketing efforts in Connecticut relative to other states might be one possible explanation of failure to detect compensation effects. We examine whether this is the case in Figure 2. It graphs weekly DMA-level advertising exposure as measured by GRPs over the entire data period for all soft drink products in Connecticut and in the non-experimental DMAs. In each panel, GRPs for all five age groups (children aged 2–5, 6–11, 12–17, adults aged 18–24 and those above 25) are exhibited. While there are large variations in these GRPs, trends are similar across all DMAs. Figure 2 seems to indicate that major advertisers in the soft drink industry, such as the Coca-Cola Company and Pepsi Co., largely operate their advertising campaigns on a national level. And while there are considerable differences in levels of advertising exposure that potential consumers in different age groups are exposed to, we see no discontinuities in the advertising exposure for any age group in the experimental DMAs around the effective dates of the bans. If anything, overall advertising exposure went down after the implementation of the ban in July 2006. This might be more a result of seasonal differences, however, as we see a similar pattern in the following year.

Turning to the regression analysis, Table 2 provides the definitions of the variables used in our regressions, while Table 3 reports the summary statistics for the demographic and marketing variables in our sample. The first four columns of Table 3 report summary statistics for households with school-age children who live in Connecticut, and in non-experimental DMAs, before and after the soft drink ban went into effect, respectively (potential treatment households). The last four columns show the counterparts for the control households, i.e. the households without school-age children.

Prices are very similar across all DMAs, with post-ban prices slightly higher than pre-ban prices on average. Advertising exposure fell between the pre- and post-ban periods in all DMAs, but seems very similar across different DMAs in both periods.[14] Households with school-age children living in

14 Marketing variables are aggregated to the DMA level and across time periods in the reported regression results such that the standard errors for these variables of the Connecticut DMA in each period equal zero.

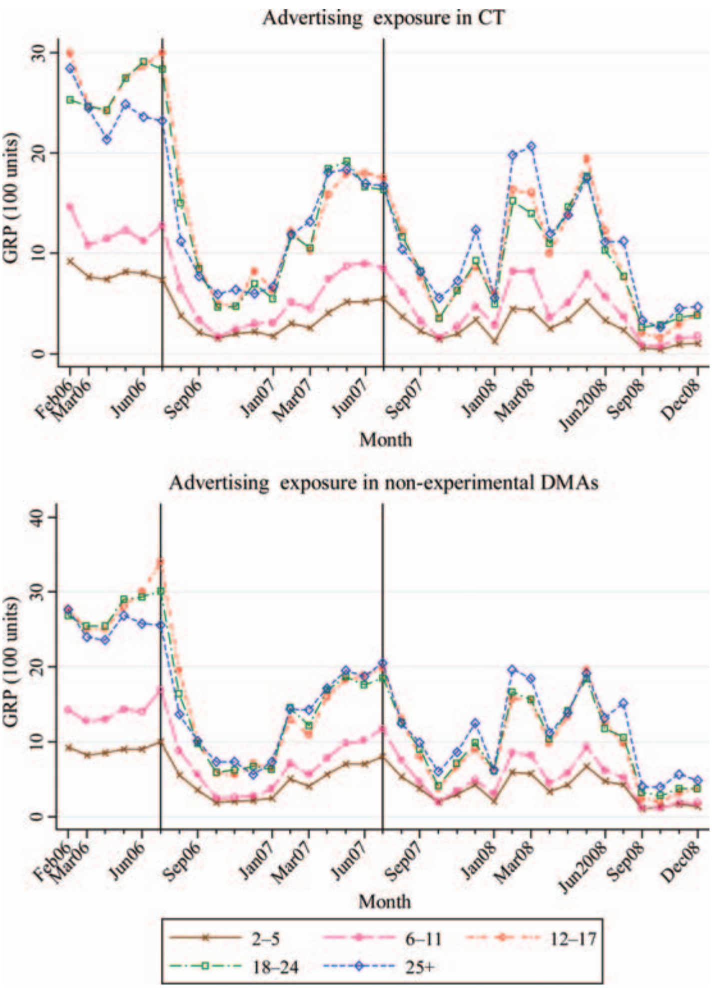

Fig. 2. Soft drink advertising exposure by age group. Notes: Depicted in the panels are the series of DMA-average aggregated monthly GRPs for audiences of the following five age groups: 2–5, 6–11, 12–17, 18–24 and over 25 for the Connecticut DMA and the non-experimental DMAs. The non-experimental DMAs are Atlanta, Houston, Miami and Kansas City. Aggregated monthly GRP is the sum of GRPs from advertising aired on cable, network, syndicated and spot television in the national market for all soft drink products. The vertical black line indicates 1 July 2006, the effective date of Connecticut (CT) ban.

Connecticut or in non-experimental DMAs are similar in most of their demographic characteristics, both before and after the soft drink ban. Households in the Connecticut treatment and control groups have slightly higher income and are more likely to be white on average, both before and after the soft drink ban compared with non-experimental DMAs. The control group residing in Connecticut or other DMAs also has similar demographic characteristics on

Table 2. Descriptions of variables

Variable	Description
Monthly household soft drink volume purchased for at-home consumption	Average monthly soft drink volume purchased by a household in a period (in ounces)
Household size	Number of household members
Low income (1/0)	1 if the household annual income is lower than USD 30,000 and 0 otherwise.
Medium income (1/0)	1 if the household's annual income is between USD 30,000 and USD 100,000
Married (1/0)	1 if the household head is married, 0 otherwise
Female has some college education (1/0)	1 if female has some college education, 0 otherwise
Male has some college education (1/0)	1 if the male head has some college education, 0 otherwise
Female is full-time employed (1/0)	1 if the female head is full-time employed, 0 otherwise
Male is full-time employed (1/0)	1 if the male head is full-time employed, 0 otherwise
Non-Hispanic (1/0)	1 if the household is non-Hispanic, 0 otherwise
White (1/0)	1 if the household is white, 0 otherwise
Black (1/0)	1 if the household is black, 0 otherwise
Home owner (1/0)	1 if the household owns home, 0 otherwise
DMA-level price (cents per ounce)	Weighted average price per ounce for all soft drink products purchased by all households in a DMA in a period. The weights are projection factors, which represent the weight of each household in the Nielsen Homescan panel
DMA-level display (in per cent)	Weighted average of in-store displays for all soft drink products purchased in a given DMA and time period. The weights are projection factors, which represent the weight of each household in the Nielsen Homescan panel
Advertising exposure (in 10,000 GRP)	Sum of GRPs across all soft drink brands and all age groups in a DMA in a period. GRP captures advertising exposure by measuring the size of an audience reached with a specific commercial. It is the product of the percentage of the target audience in the DMA reached times the frequency of views in a given period

average, before and after the soft drink ban. Trivially, households in the treatment and the control groups in either DMAs have notably different household sizes due to the presence of children in treatment households. They also have a slightly higher income and higher education level and are more likely to be

Table 3. Summary statistics for all households

	CT treatment		Non-CT treatment		CT control		Non-CT control	
Variables	Pre-ban	Post-ban	Pre-ban	Post-ban	Pre-ban	Post-ban	Pre-ban	Post-ban
Household size	4.185	4.235	4.016	3.977	1.964	1.924	1.894	1.885
	(1.236)	(1.325)	(1.201)	(1.172)	(0.909)	(0.898)	(0.844)	(0.859)
Low income	0.0988	0.0864	0.126	0.112	0.155	0.155	0.233	0.224
	(0.300)	(0.283)	(0.332)	(0.316)	(0.363)	(0.363)	(0.423)	(0.417)
Medium income	0.679	0.654	0.666	0.647	0.705	0.685	0.649	0.631
	(0.470)	(0.479)	(0.472)	(0.478)	(0.457)	(0.465)	(0.478)	(0.483)
Married (1/0)	0.802	0.815	0.769	0.794	0.534	0.526	0.520	0.512
	(0.401)	(0.391)	(0.422)	(0.405)	(0.500)	(0.500)	(0.500)	(0.500)
Female has some college education (1/0)	0.802	0.815	0.792	0.814	0.598	0.610	0.628	0.627
	(0.401)	(0.391)	(0.406)	(0.389)	(0.491)	(0.489)	(0.484)	(0.484)
Male has some college education (1/0)	0.704	0.691	0.648	0.657	0.506	0.518	0.503	0.504
	(0.459)	(0.465)	(0.478)	(0.475)	(0.501)	(0.501)	(0.500)	(0.500)
Female is full-time employed (1/0)	0.531	0.457	0.570	0.560	0.450	0.478	0.442	0.437
	(0.502)	(0.501)	(0.496)	(0.497)	(0.499)	(0.501)	(0.497)	(0.496)
Male is full-time employed (1/0)	0.815	0.790	0.758	0.771	0.498	0.494	0.419	0.403
	(0.391)	(0.410)	(0.429)	(0.421)	(0.501)	(0.501)	(0.494)	(0.491)
Non-Hispanic (1/0)	0.926	0.938	0.838	0.826	0.960	0.960	0.913	0.916
	(0.264)	(0.242)	(0.369)	(0.379)	(0.196)	(0.196)	(0.281)	(0.278)
White (1/0)	0.877	0.864	0.637	0.645	0.892	0.888	0.769	0.771
	(0.331)	(0.345)	(0.481)	(0.479)	(0.310)	(0.315)	(0.421)	(0.420)
Black (1/0)	0.0741	0.0617	0.239	0.229	0.0518	0.0518	0.159	0.160
	(0.264)	(0.242)	(0.427)	(0.421)	(0.222)	(0.222)	(0.366)	(0.367)
Home owner (1/0)	0.802	0.827	0.864	0.865	0.773	0.769	0.857	0.857
	(0.401)	(0.380)	(0.343)	(0.342)	(0.420)	(0.422)	(0.350)	(0.350)

DMA-level price (cents per ounce)	2.873	3.143	2.837	3.032	2.873	3.143	2.835	3.032
	(0.000)	(0.000)	(0.0865)	(0.108)	(0.000)	(0.000)	(0.0866)	(0.112)
DMA-level display (in %)	8.790	5.890	7.312	5.635	8.790	5.890	6.722	5.240
	(0.000)	(0.000)	(4.145)	(2.838)	(0.000)	(0.000)	(3.664)	(2.421)
Advertising exposure (in 10,000 GRP)	3.884	2.184	4.162	2.409	3.884	2.184	4.213	2.418
	(0.000)	(0.000)	(0.396)	(0.0975)	(0.000)	(0.000)	(0.375)	(0.0973)

Notes: The CT treatment group consists of households living in Connecticut with school-age children, and the non-CT treatment group refers to households with school-age children living outside of Connecticut in our data. The CT control group consists of households who live in Connecticut but without children, while the non-CT control group consists of their counterparts living in other states. For each group, pre-ban refers to the period in our data prior to the Connecticut ban on soft drink on campus that was effective from 1 July 2006, or February–May 2006. Post-ban refers to the same months in 2007.

Table 4. Simple average estimates of the impact of soft drink ban on households' at-home purchases

Location/time	Pre-ban	Post-ban	Time difference for location
Treatment houseolds: households with school-age children			
Experimental DMA	694.4	604.4	−90.016*
	(698)	(611.1)	(51.474)
	[81]	[81]	
Non-experimental DMA	572.6	461.1	−111.423***
	(636.7)	(573.5)	(20.134)
	[611]	[598]	
Location difference at a point in time	121.8579	143.265**	
	(81.372)	(71.541)	
DID	21.407		
	(54.948)		
Control households: households without school-age children			
Experimental DMA	411.4	389.7	−21.656
	(532.7)	(545.0)	(24.449)
	[251]	[251]	
Non-experimental DMA	445.5	389.7	−55.732
	(575.6)	(545.3)	(7.8704)
	[2,242]	[2,255]	
Location difference at a point in time	−34.078	−0.002	
	(35.707)	(36.214)	
DID	34.076		
	(25.629)		
DDD	−12.669		
	(64.310)		

Notes: Standard errors are reported in parentheses, and number of households are reported in square brackets.
*Statistical significance at the 10 per cent level.
**Statistical significance at the 5 per cent level.
***Statistical significance at the 1 per cent level.

full-time employed than the control group households on average. The DDD specifications control for these time-invariant and time-varying factors common to all households with or without school-age children, to all households with school-age children only and to all households living in Connecticut. It further allows us to control for unobserved factors that are common to these groups of households.

Table 4 reports simple comparisons of means for the effect of the ban on at-home soft drink purchases in DID and DDD framework. In the top panel, we compare soft drink purchases measured in ounces (oz) (1 oz = 0.296 l) of the treatment group in Connecticut and in non-experimental DMAs, before and after the implementation of the soft drink ban in Connecticut. Each cell contains the mean, standard errors and number of households included in this subsample. We see that for both Connecticut and non-experimental DMAs, there is a significant and sharp decrease in soft drink volume purchased. Specifically, the decrease is as large as 12 per cent in the Connecticut DMA and 19 per cent in non-experimental DMAs. Also listed are differences over time in the same location. Soft drink purchases do not differ significantly prior to the soft drink ban, but the treatment group in Connecticut purchases 143 oz more on average (4.23 l) than this group in the non-experimental DMAs after the ban. The DID estimate (the difference in the differences over time between Connecticut and non-experimental DMAs) is not statistically significantly different, however. The bottom panel reports the same difference for control groups living in Connecticut versus in non-experimental DMAs. Soft drink purchases do not seem to be statistically significantly different over time or across locations for these households. Finally, the DDD estimate is defined as the difference in the two DID estimates, and is also not statistically significantly different from zero.

Table 5 reports the corresponding regression results. The first two columns report the DID specifications, while columns 3–5 report the results from specifications in the DDD model. In the first column, only a constant, the post-ban dummy, and the DID treatment effect are included in the regression. In the second column, household demographics (income, employment, household size and race), prices and in-store display activities for soft drinks at the DMA level are added.[15] We also include advertising exposure of all soft drink products at the DMA level. While our advertising data report advertising exposure of different age groups including children, teenagers and adults, exposure across those groups follows very similar trends (Figure 2), introducing multicollinearity when including all advertisement exposure measures.[16] We report a regression specification using aggregate advertising exposure across all age groups, and all soft drink brands in a given DMA and time period. Finally, in all DID and DDD specifications, we control for household fixed effects and cluster standard errors at the household level.

The coefficient of the two-way interaction between Connecticut and post-ban period indicator defines the DID treatment effect in our regressions, while the three-way interaction between Connecticut, post-ban period and the treatment group (households with children 6–18) defines the DDD treatment effect. The treatment effect in the DID specification is statistically significant, indicating an increase in soft drink purchases in Hartford when compared with the other DMAs.This two-way interaction is also significant in column 5 in the DDD specification. However, once we add the additional comparison of households with and without school-age children in our DMA affected by the ban, the treatment effect switches in sign and is no longer statistically

15 The price and display are weighted averages of soft drink purchases across all products made by all households in a specific DMA during the pre- or the post-ban period. The weights used in aggregating these two variables are the panel weights in the Nielsen data.

16 Soft drink producers such as Coca-Cola voluntarily limited advertising to children under 12, placing advertisements in family-oriented programmes instead (e.g. American Idol) watched by household members in different age groups. Including advertising exposure for different age groups therefore results in collinearity, automatically dropping variables in the regression. Results for our primary variables of interest remained unaffected, however.

Table 5. Regression results of the impact of soft drink ban on households' at-home purchases

Dependent variable: monthly household soft drink volume purchased for at-home consumption	(1) DID	(2) DID	(3) DDD	(4) DDD	(5) DDD
Post-ban period (1/0)	−67.984***	22.921	−59.885***	−60.158***	31.318
	(6.979)	(58.734)	(7.450)	(7.593)	(58.407)
CT × post-ban period (DID treatment effect)	29.650	55.714**	37.837	38.354	63.351**
	(21.429)	(26.186)	(24.374)	(24.365)	(28.666)
Treatment group (1/0)			−1.319	−2.087	−2.536
			(35.939)	(37.556)	(37.679)
CT × treatment group (1/0)			52.861	51.152	51.662
			(77.120)	(75.747)	(75.816)
Post-ban period × treatment group			−38.668*	−39.751*	−42.011**
			(20.288)	(20.300)	(20.287)
CT × post-ban period × treatment group (DDD treatment effect)			−28.087	−25.315	−23.205
			(55.612)	(55.818)	(55.810)
Household size		−0.107		1.327	1.688
		(11.785)		(12.327)	(12.283)
Low income (1/0)		−35.466		−34.032	−34.394
		(36.395)		(36.083)	(36.258)
Medium income (1/0)		−11.938		−10.031	−10.853
		(30.117)		(30.092)	(30.081)
Married (1/0)		79.399		81.999	80.530
		(51.562)		(52.189)	(51.635)
Female has some college education (1/0)		−29.137		−29.652	−27.855
		(33.972)		(33.495)	(33.742)
Male has some college education (1/0)		−59.080		−58.435	−61.679
		(41.603)		(41.428)	(41.603)
Female is full-time employed (1/0)		19.827		19.667	20.105
		(25.912)		(26.058)	(25.972)
Male is full-time employed (1/0)		−29.885		−31.676	−29.552

		(31.283)		(32.018)	(31.666)
Non-Hispanic (1/0)		−53.382		−56.878	−49.139
		(51.675)		(52.381)	(51.974)
White (1/0)		40.437		40.168	40.560
		(38.649)		(39.549)	(39.262)
Black (1/0)		−17.154		−13.326	−13.207
		(42.334)		(41.969)	(42.306)
Home owner (1/0)		10.996		12.651	10.729
		(42.363)		(42.471)	(42.256)
DMA-level price (cents per ounce)		−400.840			−423.548
		(293.164)			(293.595)
DMA-level display (in per cent)		−2.866			−3.814
		(9.845)			(9.881)
Advertising exposure (in 10,000 GRP)		9.716			7.763
		(47.223)			(47.204)
Constant	473.485***	1,628.638	472.428***	505.710***	1,697.262
	(3.299)	(1,051.224)	(7.795)	(82.511)	(1,052.336)
Observations	6,370	6,370	6,370	6,370	6,370
R-squared	0.030	0.035	0.032	0.035	0.038
Number of household	3,185	3,185	3,185	3,185	3,185

Note: Robust standard errors clustered at the household level are reported in parentheses.
*Statistical significance at the 10 per cent level.
**Statistical significance at the 5 per cent level.
***Statistical significance at the 1 per cent level.

significant. The significant increase might indicate a difference in consumer preferences and overall trends for households in Connecticut when compared with households in other states, and potentially explains the early adoption of state-wide soft drink bans. Only relying on the DID estimates might therefore be misleading as we cannot account for this potential selection bias due to differences in trends across DMAs. Alternatively, the significant increase in the DID could also be driven by soft drink purchase increases for households without school-age children only. Interestingly, the interaction between the treatment group (households with school-age children) and the post-ban period is negative and statistically significant in all DDD specifications at the 10 per cent significance level, suggesting a downward trend in volume purchased for all households with school-age children in all DMAs. One possible explanation is that regulations addressing soft drink consumption at schools and the attention these policies have got, as well as potential local-or school-level initiatives, did result in an actual overall reduction for this treatment group independent of stringent state-level regulations. While we also find a significant decrease in soft drink purchases across all households in the post-ban period, this effect switches signs and is no longer statistically significant once we include controls for price, in-store display and advertising changes at the DMA level. It is worth pointing out that increases in advertising exposure significantly increase soft drink consumption in this context when we do not control for price and in-store display differences. In general, the control variables at the household-level are not statistically significant individually. While we have yearly updated information for the household demographics, it suggests that the inclusion of household-level fixed effects already captures time-invariant taste differences. Including these control variables jointly does increase the explanatory power of our regressions slightly, however. Furthermore, the results for our primary variables of interest are robust to any number of specifications including subsets of our additional controls such as including market-level controls only.

In addition, we explored a number of alternative specifications not reported here. Rather than using average monthly purchases, we summed purchases over the school semesters and used monthly purchases with additional month fixed effects. We also classified households as light and heavy soda drinkers to test whether these groups were affected differently by the ban.[17] In addition, we investigated the effect on regular versus diet soda. And finally, we investigated private label versus branded products, as soft drinks available at school are exclusively provided by the leading national-level brands. However, in all of those specifications, we fail to detect statistically significant treatment effects in the DDD specifications.[18] In summary, our results do not support the argument that

17 One might argue that heavy soda drinkers are more likely to compensate than light soda drinkers.

18 As mentioned in footnote 7, we further excluded Miami as a non-experimental DMA. The results reported here were robust to this alternative specification as well.

state-mandated soft drink bans in schools result in compensation effects in soft drink consumption at home.

5. Conclusions and future research directions

Soft drink consumption and its role as a major contributor to childhood obesity has become a highly visible public health and public policy issue. The school environment can play an important role in successfully reducing and preventing obesity in children. This study investigates the effects of banning soft drinks in schools on purchases outside of schools. It tests whether limited availability at schools results in compensation at home.

We combine purchase data with information on state-level regulations regarding soft drink availability in schools in a quasi-natural experiment approach. We use household panel purchase data and market-level information on weekly brand-level television advertising exposure directed at different age groups. Our analysis focuses on Connecticut as one of the states implementing stringent and comprehensive state-level soft drink bans in schools during our data period. By further differentiating between households with school-age children and households without children, we follow an econometric DDD approach commonly used in the policy evaluation literature.

Overall, our results do not support the argument that restricted availability at schools results in compensation at home. In our regression analysis, we are able to control for a number of additional determinants of soft drink consumption that could otherwise lead to biased results, such as possible differences in price promotions and pricing structures, as well as in-store displays. We also control for potential advertising differences and reject the hypothesis that leading brands intensify their advertising efforts to school-age children as a result of soft drink bans to offset their reduced presence in the school environment.

Our study provides a first insight into this complex topic. While our study adds an analysis of actual purchase data to the literature, we do acknowledge limitations that need to be addressed in future research. First, while our comprehensive policy review allowed us to credibly identify experimental and non-experimental markets, our study cannot address issues concerning implementation and adherence to these policies at the school level. As mentioned previously, we find an overall reduction of soft drink purchases for households with school-age children, independent of the actual implementation of soft drink bans at schools. Our failure to detect the same statistically significant effect for households specifically affected by the ban could be a result of incomplete implementation and lack of adherence to the ban at the school level, or voluntary bans in place prior to implemented state-level regulations. Contacting school districts in Connecticut and elsewhere suggested that little is known about the adherence to either state-level or school district-level regulations. Samuels *et al.* (2009) addressed this shortcoming and collected information on competitive foods and beverages available in schools for a representative sample of 56 public high schools in California in 2006 and

2007. Focusing on the adherence of mandatory nutritional standards, they report that California schools are making progress towards full implementation. While beverage standards seemed easier to achieve than standards for food items, soft drink availability still varied significantly across schools surveyed in their sample. A future research extension to this study will analyse purchase response to the ban implemented in California high schools by combining this unique data set with store-level purchase data from a major retailer for all California stores covering an extended time period. Matching stores to neighbouring schools with diverse adherence measures will allow us to directly address this important aspect.

Another limitation of our study is that we do not observe restaurant purchases, especially soft drink purchases at fast food restaurants. Students might compensate by increasing their purchases at those outlets, which would result in underestimating the compensation effect in our data. However, it seems plausible that students would at least partially compensate through purchases in the outlets included in our data set.

Previous research suggests that banning soft drinks decreased calorie consumption at schools (e.g. James *et al*., 2004; Blum, Jacobsen and Donnelly, 2005; Fernandes, 2008; Schwartz, Novak and Fiore, 2009; American Beverage Association, 2010). If these findings capture a general trend, and are applicable to the schools in our sample, our results suggest that banning soft drinks at schools does not result in compensation at home. Our study further supports the notion that soft drink bans at school reduce overall calorie consumption from soft drinks in children. As such, our results inform the policy debate on successful strategies to reduce and prevent childhood obesity. We suggest that soft drink bans at school present a potentially effective policy option in this regard.

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